

July 14, 1952

Dear Aaron:

As you have probably guessed by now, we did not stop at Chicago this last weekend trip. We drove straight down to Urbana and returned similarly, via Beloit. It was just too damned hot for anything more ambitious. We are however anxious to see you, although do not at the instant see a suitable opportunity. I hope your forthcoming bundle of fortune will not immobilize you unduly. Bruce told us a bit about your anti-mutagens— it sounds very exciting. I don't have any but the most obvious ideas about it. The possibilities of ~~kinetics~~ the analysis of the change-over period from one rate to another sound very good. Why hadn't we thought of using the temperature influence in the same way, or would it be too complex. Are your mutagenic and antimutagenic effects competitive? Substances which are nearly indifferent in their effect on the spontaneous mutation rate might antagonize the effects of added mutagens or antimutagens.

We were led down the garden path by a minor incident or accident that may also bear on your work. About June 1948 we received a set of phages from you, including T5. As it happens, we had very little occasion to use T5, and when we did must have used mostly another stock of it sitting in our refrigerator (probably grown from a sample from Luria). But recently, we got to use yours, sitting in the original vial as received. This stock is not T5, as we eventually discovered. I don't have any idea how or when the contamination arose; it does not seem likely that it happened after we received the vial, but some gremlin might have poured out the original and replaced it. We're pretty well straightened out again now, and don't care to go into it any further, but wondered if you might possibly have rediscovered the same thing with your stock, or if you might have the same trouble yourself. I have not identified what phage is in ~~that~~ the "T5" vial. It does attack cultures labelled /1,5 (e.g. B/1,5 and our various V_1^r in K-12 [including W-1177; Y-40...]). Our bonafide T5 gives the proper reaction to these cultures, and to the B/1 types.

We went down to Urbana mainly to talk to Joe and Lu about lysogenicity. Their stuff and our crossing work fit together beautifully to suggest that the provirus in lysogenic bacteria actually sits on the chromosome. If we had some decent lambda mutants (aside from the lytic one), we could tie it together much more satisfactorily, but the non-concordance of technical desiderata in single strains seems to be an important evolutionary law.

Our best wishes to the dotting pro-parents.

Sincerely,

Joshua Lederberg